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SECTION OF GEOLOGY AND MINERALOGY

FEBRUARY 5, 1940

LIEUT. PAUL A. SMITH, U. S. Coast and Geodetic Survey, Washington, D. C.: *Exploring the Continental Shelves and Slopes.* (This lecture was illustrated by lantern slides.)

Knowledge of the topography of the sea floor has grown rapidly during the past decade, largely through the development of echo-sounding. Due to the accompanying need for more complete nautical charts, several of the maritime nations have extended hydrographic surveys seaward from their shores and, due to this seaward extension of the surveys, a whole new physiographic realm is being opened to students of earth science. The perfection and use of offshore position-finding methods and echo-sounding by the U. S. Coast and Geodetic Survey have added many thousands of square miles to the surveys of the continental shelves and slopes of United States and waters under their jurisdiction. The discovery of a stream-erosion type of topography on the seaward faces of the continental shelves under hundreds of fathoms of water has raised questions that seriously affect a number of cherished geological theories. While topographic characteristics of numerous submarine canyons are

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clearly and unmistakably those commonly observed in valleys eroded by running water above the sea, many geologists have felt that the simple explanation of a lowered sea or rising land in the order of 10,000 feet, in late Tertiary or Pleistocene time, is untenable in the face of apparently well-established theories. A number of hypotheses have been advanced in the attempt to explain these features by some process that may harmonize with other geologic evidence, but the origin of submarine canyons is still controversial.

Through a number of contributors including Dr. H. C. Stetson of the Woods Hole Oceanographic Institution, Dr. Maurice Ewing of Lehigh University, the Carnegie Institution, and collaborators, and Dr. C. S. Piggot of the Geophysical Laboratory, our knowledge of the materials and underlying strata of the sea floor is being expanded. In all of this work, the Geological Society of America has been especially interested, and has contributed greatly to the growth of knowledge in this field of science through grants to various investigators.

Automatically recorded profiles of submarine topography of the Atlantic Continental Slope show decidedly V-shaped cross sections, in the extreme heads of the canyons as well as in depths of 6,000 feet and over. Due to certain limitations of echo-sounding, it is sometimes impossible to record the bottom of steep, narrow ravines in deep water, or to record true slopes in very rugged topography. Graphic records of echo-sounding always give a more complete picture of the profile than visual indicators and, in many instances, reveal phenomena that might otherwise be missed, such as the great sand waves found south of Nantucket Island, during the summer of 1939, by the Coast and Geodetic Survey ship *Oceanographer*.

The abrupt change from the topography of the shelf, which is plainly marine-made in character, to that of the slope which shows no indications of marine erosion, but is plainly a type of topography commonly attributed to stream-erosion,

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is one of the most remarkable of the many interesting facts demonstrated by the new surveys. The coöperation of the Geological Society of America through the late Dr. A. C. Veatch and the Coast and Geodetic Survey has resulted in the publication of these new surveys in the form of special charts for use by various investigators.

SECTION OF PSYCHOLOGY

FEBRUARY, 19, 1940

PROFESSOR JOHN G. JENKINS, University of Maryland, College Park, Md.: *The Questionnaire as a Research Instrument.**

It is a happy state of affairs that certain psychological problems lend themselves to attack by methods of laboratory experimentation. Although we have had to learn that it is ordinarily vain to talk of single variables in psychological research, it is none the less possible, in many cases, to reduce the number of variables to a point where one may safely speak of an outcome of measured dependability. Where this is possible, psychology need not fall back upon more questionable methods and, indeed, there are those who would insist that psychology has no right to venture beyond the limits of laboratory experimentation.

A somewhat larger group of those who call themselves psychologists has held that this point of view eliminates from the field a great many basic problems of human behavior. Accordingly this group has not hesitated to make use of tests and scales, and has even been willing to employ that methodological scapegoat—the questionnaire. It may be that there are among my listeners tonight those who are irrevocably convinced that the questionnaire is a hopelessly bad tool which should be summarily ejected from the research equipment of the psychologist. To such I can only offer the humble bow of an Untouchable in the presence of a Brahmin. There may be others who would hesitate to use a questionnaire unless evidence of improvement could be adduced. To such I hope to offer words of encouragement. And doubtless there are those among you who have repeatedly made skillful use of questionnaires in the past, are using them ably now, and will continue to employ them on a high plane in the

* This lecture was announced under the title "Psychological Research on Aviation Problems."

future. To you I can offer only apologies for boring you by pitching my discussion on an elementary level and concerning my exposition with matters that have long been familiar to you.

The acceptance of the questionnaire as a research device varies over an impressively wide scale. If it would not be amiss, I should like to suggest that this variation in the professional attitude toward the questionnaire is to some extent a function of the widely varying degree of sophistication regarding the methodological development of this instrument. For a decade I have made it my business to look critically at all questionnaires that have come into my hands and to talk to psychologists regarding their understanding of the preparation, use, and interpretation of the questionnaire. As a result I am convinced that no other research device is employed today on so widely varying a scale of excellence as this one. It is granted that *experimentation* can be sloppy and careless, that *statistics* can be naïve and ill-founded, and that *logic* can be terribly warped by the tyro. Yet all of these are fairly readily spotted in publication, even by psychologists of moderate training. With regard to the questionnaire, however, the situation is different. It is easy to point to men whose experiments, statistics, and logic are above reproach, but who have employed questionnaires apparently in blissful ignorance of the fact that this device must meet certain methodological criteria.

Surely you will not question that—at one extreme of a distribution—we find a questionnaire all but entirely free from systematic virtues. Here we have the all-too-familiar case of the questionnaire formulated on an *a priori* basis in the seclusion of the study with no regard for question-form or question-order, circulated through a sampling defined by expediency and interpreted by a triumphant pointing to those gross totals which were pre-determined by the bias of the inquiry. It is scarcely necessary to add that questionnaires of this type will probably seek information that has already

been made available in much more accurate form by precise quantitative experimentation or will attempt to secure vague generalizations regarding subjects concerning which the respondent cannot possibly have accurate knowledge.

By way of specific example, I present in evidence a questionnaire sent me during the current year with the request that I should have 500 of our students fill it in. It was distributed by an organization notably interested in interfering with the convivial habits of those who regard the ingestion of solutions containing ethyl alcohol as a social good. It may be possible to typify the entire inquiry by quoting the first question which read, "Do you ordinarily behave in a more ethical fashion after drinking large amounts of alcoholic liquor?"

If questionnaires of this sort were circulated only by isolated pressure groups interested in special pleading for a foreordained result, the matter might be of no great concern to any psychologist. The cold fact is, however, that the passing years have been marked by the receipt of a number of questionnaires issued by professional colleagues and marked by an internal organization on a level little higher than that of the interrogatory just mentioned. The speaker feels quite strongly that this situation is to some extent a result of the fact that the improvements of various aspects of questionnaire technique have been made in widely separated fields, and that parochial publication has confined knowledge of these improvements largely to the specific field in which they originated. Once one begins deliberately to search through these various fields for information on refinements of common technique, one finds a truly amazing bulk of material, all of which points in the direction of better control and increased accuracy of outcome.

It is perhaps appropriate to confess that an appreciation of this situation was forced upon me in an odd fashion. Several years ago, I discussed this general thesis with the editor of the *Psychological Bulletin,* who thereupon invited me to

contribute an article which would attempt to bring together from widely separated sources available items of information with regard to the use of the questionnaire as a research instrument. At the time, I must admit, I regarded myself as quite sophisticated in the matter and anticipated that the preparation of this general review would occupy whatever time might be available for the next month or two. I know of no better way to characterize the situation than to remark that, although every spare moment has been devoted to the task for more than three years, the review is only now beginning to take shape in a preliminary draft. Instead of finding, as anticipated, that it was merely necessary to transfer from a card file a digest of materials already covered, events proved that almost every article reread led further afield into sources which had been blandly ignored in a less specifically motivated search through the cognate literature. I should hesitate to boast that the end is even now within sight.

To summarize very briefly what has been forced upon me by a review of this heterogeneously located series of publications, I should like to offer a few categorical statements. These statements may then be elaborated and defended as you sit in judgment upon them. I shall ask only that you attempt to defer decision until the discussion is complete.

The statements which I should like to defend are as follows:

1. The questionnaire may properly be used only for the study of unique events and of attitudes and opinions. In circumstances which set a premium upon economy of time and money, however, it is sometimes defensible to use it for the collection of certain other types of data.

2. No attempt should be made to formulate the questions to be used until the author has had time for an extensive preliminary personal investigation of the field to be studied.

3. No questionnaire should ever be used for actual investigation until it has been extensively pre-tested on samples of the population to be studied in the final research.

4. The formulation of the individual questions and the determination of the physical form of the questionnaire should be carried out in the light of available research-information on the way in which these factors affect the results obtained. The investigator must necessarily bear in mind that the response is a function of the form and the position of the question.

5. Interpretation of results cannot justifiably end with the mere scanning of gross totals. Search for covert content and the use of refractionation are vital necessities for significant interpretation.

6. Interpretation of the results should always be made with the realization that the results obtained apply only to those who responded to the questionnaire. It is never defensible to extrapolate from actual respondents to the full sampling.

7. The study of motivation by the use of direct WHY questions may ordinarily be expected to yield only a rationalized account. It is possible, however, to obtain valuable insights by the use of a program of interlocking indirect questions, especially if the questions are laid out along the time-line of the original response.

Turn now to these statements in order. The first statement obviously contains three parts, each of which should be considered separately.

In pointing out the usefulness of the questionnaire in studying unique events, I am heartily indebted to Truman Kelley[1] and I shall not hesitate to use his weapons in defending myself against those purists who would insist that all events are unique. True it is that no combination of hydrogen and oxygen in a test-tube is ever precisely the same as any other, strictly speaking. But the combination of hydrogen and oxygen in a test-tube has the characteristic property of *repeatability*. Turn to psychological realms, and you find many problems that are repeatable, and hence just as prop-

[1] Kelley, T. L. *Scientific Method*, 1–38. Macmillan. 1932.

erly subjectable to experimental investigation as the synthesis of water. We may run the memory drum over and over for many subjects; we may repeatedly induce nervous breakdowns in rats; and we may photograph eye-movements hundreds of thousands of times, if necessary. These *repeatable* events are the natural subjects of experiment and there is no adequate reason to attempt to study them by means of any instrument which involves a larger probable error. Thus, the probable error in any laboratory study of the effects of alcohol is large enough to dismay anyone. To attempt to gauge this effect by means of direct questions increases the probable error to astronomical proportions.

On the other hand, human behavior abounds in unique events. It would appear that psychology must either refuse to consider them—and there are those who would do that—or accept some method which, in spite of a larger probable error, casts some faint light on the situation. You may either ignore the fact that I spanked my son last week, or you may ask about it. That particular spanking-situation is not repeatable. You may pay no attention to the man taking his first airplane flight, or you may ask questions about it. You may close your eyes to problems raised by the loss of a job, or you may ask a jobless man about it. It is the thesis of this paper that social psychology, for example, need not deny itself materials of this sort, provided it understands that the accuracy of the information obtained is a function of the refinement of the instrument employed.

The situation is the same with regard to obtaining information regarding opinions and attitudes. If you hold firm to the idea that opinions and attitudes are not important for psychology, or if you believe that *directly observable* behavior holds adequate representation of opinions and attitudes, then the problem of the questionnaire need not bother you. If, however, you doubt that the situation is so comfortingly simple, then, to obtain the information you need, you must ask questions. Questions may show that a man who votes

the straight ticket does so under political pressure and holds attitudes quite at variance with his voting behavior. Inquiry may reveal that the professor who acquiesces in faculty meeting actually conceals the storms within his soul. Questions may or may not reveal what is concealed. We shall presently suggest that *direct* questions will probably lift few curtains but that a program of *indirect* questions, formulated in the light of modern knowledge of methodology, may open doors so gently that the respondent himself is unaware of revealing anything.

Finally, although the privilege is often abused, the questionnaire may be used to obtain certain types of factual data when the situation sets a premium upon economy of time or money. It would be possible, for example, to travel and to learn by direct observation how many hours per week psychologists are required to teach, but the cost would be exorbitant. Adequate results can be obtained by a properly sanctioned questionnaire. Again, it would be possible, by direct observation, to determine the laboratory equipment of the 40 largest laboratories, but again the expense involved justifies the use of a properly sanctioned questionnaire.

Now, nothing that has been said is intended to deny or to condone the abuses that all too often characterize the use of the questionnaire, even within the fields named. There is little reason to believe that defensible results can be obtained by inquiring about unique events or attitudes that lie far in the past. I shared the impatience of a 65-year-old colleague when he was asked what his interests had been at the age of 5.[2] Likewise there is no defense for questions which seek information not actually at the command of the respondent.

[2] At the meeting at which this paper was presented, J. C. Flanagan reported that Rulon had obtained evidence of high accuracy when he asked members of a 25-year class to report on their interests as undergraduates. The reference is not available in standard psychological sources. Even such a demonstration would be highly specific in nature and in time; and, in view of all that has been brought out in psychological studies of testimony, the burden of proof would always rest with those who would assume the accuracy of inquiries about the long past.

Only recently I was asked to estimate the amount spent annually on liquor by the average undergraduate at my university. If I had responded, the meaningless numbers I set down would doubtless have been thrown with others and averaged to several decimal places. This is a species of professional malfeasance that is below contempt. And we should have only severe censure for those who lazily attempt by questionnaire to obtain information more accurately available in other sources. There is no reason to estimate by questionnaire the percentage of APA members holding the Doctorate when the annual directory readily supplies an accurate answer.

It would appear that the first rule of questionnaire construction should be to study the appropriateness of the method to the problem at hand.

If you will grant at least the major emphasis of the first statement, we may turn to consider the second. This, you will recall, read about as follows: "Only after adequate time has been allotted for a preliminary personal investigation of the field under consideration should the questionnaire be formulated." Here is a point on which the divergence between excellent and poor questionnaires is wide indeed. A certain Ivory Tower tradition has led to the growth of the idea that the psychologist, being a capped and gowned Prober of the Mysteries, needs only the esoteric activities of the Little Gray Cells in order to turn out an effective questionnaire in any field. (Would it be impertinent to remark that the present generation of psychologists shows much less of a tendency to don the long robe and to play the savant? Or have you forgotten the academic atmosphere which led an American psychologist to write, three decades ago, in terms of a Doctor Watson in the form of a layman who always marvelled at things mysterious to him which were as an open book to the trained psychologist?)

The field of psychological market research has played a dominant rôle in showing the importance of the informal in-

vestigation which precedes the formulation of the first question. We have many trained psychologists in this field today and none would venture to make up an arm-chair questionnaire. They have had to learn that a brief trip into the field is always necessary to find out what to ask, how to ask it, what words to use, what questions will embarrass the respondent—in brief, to get the "feel of the situation." In this preliminary investigation, too, one frequently sets up hypotheses which serve to shape the whole course of the investigation. Leave out this stage and you will surely find yourself with questions which are unnecessary, questions which are worded improperly, set in terms which are either obsolete or ambiguous, and you will quite likely find that your beautifully spun hypotheses—which looked so fine in the quiet of your study—are pitifully inadequate in a world of real men and real events.

Let me offer an example or two from the field of marketing research. How was a city boy—even though a psychologist—to know that a study of material for admitting ultraviolet rays to hen-houses would necessitate a question as to whether the windows were washed and, if so, how often? Or, how would the same city boy know without preliminary study that most of his respondents would think that the word "peer" meant *superior?* Or, if you were invited to study why Community Chest returns fell off in a given city, how would you know what questions to ask, until you had chatted with the local folk to get some leads? Or, if you were sent to discover what laborers in the steel mills thought of unions, how could you possibly phrase your questions for them without preliminary study right among the steel workers?

Fortunately, signs of using the preliminary investigation are not limited to the technological fields. Some of you doubtless remember a paper given at Minneapolis by Augusta Jameson[3] in a study of the development of delinquency in

[3] Jameson, Augusta. Psychological Factors Contributing to the Delinquency of Girls. *Jour. Juven. Res.* 22: 25–32. 1938.

girls. If so, you will recall that she first went to the institution in which the study was made and spent several months—not days—simply acquainting herself with the girls and with the world in which they lived. Only after her stay there had been long enough to gain her acceptance into the group, did she begin to ask their aid in phrasing her questions. The final questionnaire, phrased in terms designated by the girls themselves, and touching upon matters of importance discernible only under direct investigation, could be relied upon to gain information that would never have been obtained by an instrument phrased in collegiate terms and aimed at matters assumed *a priori* as important by an academician.

It is obvious that this second statement leads naturally over into a third, which reads: "No questionnaire should ever be used for the actual investigation until it has been extensively pre-tested on samples of the population to be investigated." Now the man who will design a questionnaire in the silence of his study, will usually disdain any field test —yet even the remotest acquaintance with the results of such a test cannot fail to carry conviction. In actual practice, the pre-testing of a questionnaire corresponds closely to the period of experimentation known colloquially as "getting the bugs out of the apparatus." As a result, it is not uncommon to spend months pre-testing a questionnaire which may achieve its final usefulness in a few weeks or even days.

The series of pre-tests will detect ambiguous and unclear questions. It will show unforeseen effects of one question upon another and indicate necessary changes in question order. Almost inevitably it will show that certain questions are unnecessary and that other important questions have been omitted. It will indicate changes in wording and changes in methods of recording the answers. In fact, it will most embarrassingly show one how feebly the human intellect can function in preparing an *a priori* analysis of a complex situation. In the field of market research, a series of pre-tests has become mandatory and the bulk of evidence on its effects

has become monumental. In certain fields of socio-psychological investigation, alas, it is almost unknown.

You will appreciate that it is difficult to bring in specific evidence on this point, except by way of example. Very recently, for example, our staff took part in a study of the sale of photographic materials. Seeking only to study the acceptance of polychromatic pictures, we asked whether the respondent took colored pictures more often than plain ones. Looking back now, I see readily that certain negro respondents might have taken the word "colored" to refer to ethnological rather than to chromatic differences and have responded accordingly. I did not see it in advance, but a pre-test brought it out. A pre-test also showed that most of our poorest respondents thought that the General Electric Company generated the power for their lights at home. Another pre-test revealed that college freshmen were quite unable, without personal assistance, to answer the question: "In what circumstances did you reach the final decision as to what college to attend?" (We were forced to grant the palm to one student who answered, quite justifiably, "In very poor circumstances.")

In a word, the questionnaire, as it comes from the platen of the author's typewriter, may be clear, unambiguous, unbiased, and unembarrassing only to the author. Preparing it for effective work in the field means pre-testing it—not with available and long-suffering graduate students—but in the field and with a sampling of the final population. Such a pre-test inevitably leads to revisions which must then be subjected to a further pre-testing. In the speaker's experience it is a rare questionnaire which will be ready for use with less than a half dozen revisions and pre-tests.

Pre-testing points to the necessity for phrasing the questions in terms of the requirements of the respondent. Our fourth statement suggests that wording and physical form must be aligned also with rules developed in the course of psychological research. The study of the effect of the form

of a question was not new when Muscio[4] worked experimentally on it more than two decades ago or when Burtt and Gaskill[5] tackled it in the laboratory some time later. The mere inclusion of a definite article has been shown to influence the response. Likewise the change from a specific to a general form has been shown to have noteworthy effects. More recently, we have seen evidence that the inclusion of a famous name as the sponsor of an idea will materially change its acceptance. And we have seen that the attaching of a label may alter the very sign of the response, as when those who indignantly reject a doctrine labelled as fascistic, enthusiastically embrace it when presented without any label. He who formulates questions only stores up trouble for himself if he works in ignorance of these elementary facts.

Go beyond these basic principles and you come to many other available guides for the shaping of questions. Ghiselli[6] has shown that providing for four possible responses, instead of a simple yes–no alternative, may alter not only the proportion, but, as well, the actual balance of opinion. This is one of many links in the chain that would restrict the use of yes–no questions to situations in which one has to choose only between mutually exclusive alternatives. Much work that has been done on rating scales and testing techniques may profitably be carried over as a bulwark against those who would unjustifiably make of the yes–no question a veritable Procrustean bed in which human attitudes are trimmed to the taste of the interrogator.

The yes–no type of question, one suspects, is introduced largely because it lends itself so conveniently to scoring. Very likely the same motive has led to the popularity of the multiple-choice or check-list type of response. The speaker

[4] Muscio, B. Influence of the Form of a Question. *Brit. Jour. Psychol.* 8: 351. 1916.

[5] Burtt, H. E., and Gaskill, H. V. Effect of the Form of the Question. *Jour. Appl. Psychol.* 16: 358. 1932.

[6] Ghiselli, E. E. All or None versus Graded Response Questionnaires. *Jour. Appl. Psychol.* 23: 405–413. 1939.

began to suspect this form when a result encountered in a consulting capacity carried with it an odor that was unmistakably piscatorial. An effort was then made to determine possible results in the event that the list of items was not exhaustive when presented to the respondents. Some of you may recall that a test[7] was made with several hundred students. One group was given a free-response question and, on the basis of frequency of their replies, a check-list was made up. Although the order of items was scrambled, the values received on the check-list coincided closely with those on the free-response. Then the check-list was mutilated by the deletion of the five most popular answers and the list was presented to a third group, with the request that they should supply any items they wished. No longer were the results similar. The other items moved well up on the list and the five original top items were barely mentioned. The general result, I may say, has since been verified in quite a different setting.

Such studies have led to the formulation of a general principle. A check-list is satisfactory for the comparison of a limited number of known possibilities, but it should not be used to explore a field in which all of the possible answers may not be represented on the list.

The quest for "objectivity" has led to the use of two other mechanical devices—ranking and rating. The potential evils of the first are so apparent as to require little comment. Any attempt to average ranks assigned to the several answers implies a tacit assumption that the ranks are separated by equal psychological distances—an assumption rarely if ever justifiable. It involves one in the mathematical sin of treating ordinal numbers as if they were cardinal numbers—a sin encountered in the litany of grade-school and, unfortunately, early forgotten. The speaker was once in a position to demonstrate to a commercial group what this meant, for the

[7] This has never appeared in the periodical literature but is described in: Jenkins, J. G. *Psychology in Business and Industry.* 348–351. Wiley. 1935.

respondents had been forced to comment on the meaning of their rankings. These comments showed clearly that for one respondent, No. 1 and No. 2 were almost ties, while, for another, No. 1 was infinitely far above No. 2—and so on. Yet in the report where these figures were presented, these ranks had been solemnly averaged to the second decimal place. Where the proponents of the device have sought to use it for ease of interpretation, the speaker would say that one should avoid the use of ranking in view of the practical impossibility of telling what the ranks mean.

Nor does rating fare much better, when critically regarded. Quantitative estimates of this sort may be easy to give, but their validity is dubious to the extreme. Within the past ten days the speaker has been asked to pass upon a questionnaire in which he was called upon to rate the percentage of time in which he was characterized by each of a large number of adjectives. The author of the device insisted that any intelligent adult should be able to estimate, within a small margin, whether he had been "honest" let us say, 80 per cent of the time or 70 per cent of the time since birth. If this be so, I must confess that I fall short on both intelligence and adulthood. In any event, the author, in claiming high validity for his instrument, based his claims of validity upon the fact that undergraduates found it easier to fill out than any other form. This, I take it, is validation by expediency—a new methodological concept!

When evidence on question-forms is examined, we are faced with this general situation. The use of yes–no questions is approved when all that is asked is a choice between mutually exclusive alternatives. The multiple-choice form is approved when evidence is shown that all possible choices are represented. Doubt is cast upon the desirability of rating and ranking. This means that, in most cases, the free-response form is indicated, and this, indeed, must be formulated in the light of all we know about how the question influences the answer. Free-response questions may be inconvenient

to tabulate, but, if properly worded, they do not warp the response by their own mechanics.

The study of free-response questions includes, of course, all of the materials available from the many studies of the psychology of testimony. It must be expanded as well to include a search for questions that are "loaded." A questionnaire recently laid in my hands asks: "Do you enjoy the luxurious ease and personalized service of travel by air?" It involves as well a search for questions that are perfunctory, that put a premium upon dishonesty, or that require answers interdicted by social usage. It involves a realization that questions that would be embarrassing must be rephrased and that a series of indirect questions may succeed where a single, direct question would fail. And, above all, it involves a certain knowledge that asking the same question from different angles permits a series of checks upon internal consistency that would otherwise be lacking.

Finally, of course, the formulation of questions rests upon the realization that the respondent always answers a question-in-a-setting and never the question-as-phrased-by-the-author.[8] Chesterton pointed this out in one of his Father Brown stories many years ago, and anyone who has worked in the field with actual respondents has encountered it many times. Thus, if I ask one of you, "Are you doing anything after the lecture tonight?" you may say "No," ignoring the fact that you plan to take the subway, go home, order the milk for tomorrow, read a short story, and then revise six pages of an article while trying to go to sleep. Your "No" refers to activities which fit into the context of my probable invitation to share in a discussion at a nearby restaurant. It ignores the routine activities in which I probably have no interest. There is perhaps no better indication of the truth of this statement, than the social rating we assign to the intolerable bore who

[8] This is one of several pertinent points treated in: Roslow, S., and Blankenship, A. B. Phrasing the Question in Consumer Research. *Jour. Appl. Psychol.* 23: 612–622. 1939.

insists on replying literally and in detail to our polite inquiry "How are you today?"

In our social life of everyday this tendency may be a life-saver. In questionnaire studies it poses a very difficult problem. Lazarsfeld[9] has pointed out that many respondents approach a question with some *tacit presupposition,* and that this presupposition may vary from person to person. Realization of this means that we must bend every effort in formulation and in pre-testing to discover and to care for possible contexts which may cause widely differing interpretations of our questions. No general rule is possible. Much hard work in the field may, however, go a long way toward securing a uniform interpretation of the question as asked.

If recent years have brought improvements in the form of the questions used, they have brought even greater possibilities of improvement in the techniques of interpretation. It is not difficult to turn back only a decade and to find many examples of questionnaires which were skillfully constructed, distributed to an adequate sampling, and then set into publication with no interpretation beyond the gross scanning of totals. The well-trained specialist in this field today knows that his work is only well begun when the totals of the several columns are available. There are two major tasks which lie before him when the cold figures are finally available. The first of these is the search of the data for covert content indicative of a theory or principle which may in itself immediately require further investigation. The second is a patient process of fractionation and re-fractionation of the raw data in search of relationships which, although otherwise concealed, may well provide the most important outcomes of the investigation. Let us consider these in turn.

We may illustrate the first by turning to an extremely important publication which has just come from the Fatigue Laboratory of Harvard University. As most of you know,

[9] Lazarsfeld, P. F. The Art of Asking WHY in Marketing Research. *Nat. Marketing Rev.* 1: 1–13. 1935.

Elton Mayo and his associates have devoted a decade of intensive study to certain aspects of the personnel situation in a plant of the Western Electric Company. (The study began, you will recall, when a more or less routine investigation of the effects of rest-pauses failed to obtain the results that were expected.) In the most recent report of this long-time study, Roethlisberger and Dickson[10] describe the application of the questionnaire technique to a study of the complaints of a large number of employees. A few years ago, I suspect that the end of the study would have come in the form of a frequency-tabulation of the complaints made by the workers. The results, however, compelled these investigators to adopt the point of view that such a tabulation would have little meaning —that is, that the actual items complained of were not in themselves significant causes of difficulty. This led in turn to a detailed consideration of the social organization informally and undeliberately set up by the workers and showed that offenses against this social structure were the real causes of unrest. (In passing, I am impelled to say that, if this book is not an important highlight in social psychology and in labor economics, it will be for reasons beyond the speaker's present comprehension.)

The example, at any rate, is clear-cut. Take the frequency tabulation in itself, rectify the items complained of, and you have failed to change the situation significantly. The tabulation takes on meaning only as it is seen against the background of a unifying theory provided by the covert content of the responses. The same idea was well expressed by a visiting psychologist who once told one of my classes: "Professor Jenkins has shown you how beautiful tables and charts may hide the real facts. He has shown you how to make analytical tables and charts. Now I tell you, throw them all away and make instead a theory." (And to make his point, he showed the class a beautifully unifying theory for the very

[10] Roethlisberger, F. J., and Dickson, W. J. Management and the Worker. 255–269. Harvard Univ. Press. 1939.

data over which we were working and which I, in my enthusiasm for mere refractionation, had overlooked.)

It will be clear to all of you that such theories are not to be built by idle speculation—that they must be *empirical* theories. Now, one of the greatest aids in building up an empirical theory is precisely the fractionation and re-fractionation already mentioned. On an early consulting job, for example, I was faced with beautifully drawn graphs which showed that the sponsoring company was obtaining a very large share of the market for a group of products. The conclusion to be drawn from such graphs was unmistakable—the current policy of the company was above reproach. Fortunately the raw data were available and we began to re-fractionate them. The very first check showed that, although the company was selling more than its expected share to very large buyers, it was selling much less than its expectancy to small buyers. A second fractionation showed that the geographical distribution of sales was by no means uniform. And a third indicated that sales varied considerably with the nature of the mechanism for purchasing. Instead of a confirmation of the existing policy, we were faced with a very much more complex interpretation of the situation.

In market research situations this is so generally the rule that no research group today would venture to stop at the scanning of gross totals. And it is not difficult to show that the same situation obtains in many other fields of research. In one study of college-attendance, for example, the gross totals themselves would have indicated conclusions which were greatly at variance with those obtainable when the results had been successively broken down by economic level, by geographic origin, by college registration, and by types of institutions considered. Likewise a recent study of faculty attitudes with regard to establishment of academic policy would have fallen far short of the mark, if only totals had been considered. Fractionation of the results indicated a wide disparity between certain well-defined groups indicated

by breakdowns according to age, to academic rank, and to length of service. The process of fractionation is vastly aided if the material is coded and punched onto cards for machine sorting, but the difficulties of hand sorting may be well justified by the results obtained where machines are not available.

The process of interpretation involves us at once in the whole theory of sampling and in the problem of how far we may go in extrapolating from our sample to a larger universe. I shall not bore you by going into the mechanics of obtaining representativeness and reliability. Suffice it to say that anyone of serious intent can estimate in advance the size of sample he will need to obtain results within a given region of tolerance and then check it progressively in actual application. Sampling theory and practice are well enough developed today so that there is no longer any excuse for some of the gross inaccuracies that are observable in the distribution of current questionnaires.

It is not difficult to show that many who hew to the line with regard to the representativeness and reliability of their sampling blandly ignore the more important problem of whether they are justified in extrapolating from those who *do* respond to those who *do not* respond. Relatively little direct evidence is available on this point, but the evidence that is in, definitely suggests that the burden of proof rests on those who assume that the addition of responses from those who did not answer would not have changed the picture. This problem arises not only in mail questionnaires but in face-to-face inquiries as well. One study of the former type of inquiry was made by W. J. Reilly[11], who conducted interviews with all of those who had received a questionnaire sent out by an advertising agency. A comparison of replies made by those who had returned the questionnaire with those who had not indicated that, on every matter considered, the responding group was not typical of the entire sampling. Most

[11] Reilly, W. J. Checking upon Data Secured by Mail Questionnaires. Printers Ink. 71. Dec. 23, 1926.

important, perhaps, was his discovery that the responding group was definitely more favorable to the product than the group that did not respond. A similar study involving a very careful and persistent effort to cover every potential respondent in two cities yielded results that indicated an even greater disparity between the results obtained from those who responded and those obtainable from the total sampling.

Although the results will always be specific to the situation, many other studies of this sort are needed. If, for example, it is found that the general trend of those who do respond is more favorable than of those who do not, some allowances may be made in interpreting later results. If, as certain findings suggest, the results of those who do respond are more accurate than those which would have been obtained from the total sampling, the fact is surely not without significance. If the ready respondents generally show a greater homogeneity, we should be able to discover this; and if there are characteristic differences from field to field of investigation, the more rapidly the facts can be uncovered, the more we can do to reduce our probable errors. In the meantime, it seems highly necessary to bear in mind that, unless a 100 per cent response is obtained, the results can properly be regarded as shedding light only on those who did respond.

My final—and to me the most significant—statement has to do with the study of motivation by means of questionnaires. I am sure that I represented only the common trend in the field when I told my students 15 years ago that the questionnaire was utterly useless for the study of motivation. It was easy to argue that inquiring about motives yielded only rationalizations, that it obtained "good" motives instead of "real" motives, and that it gave us, at best, only someone's opinion of the forces which motivated him. It was easy to hold up horrible examples, to demonstrate that inquiry reduced the splendid complexity of human motivation to an artificial simplicity. By way of example, I recall, I adduced a popular list of "basic motives" which had been made up by asking student groups to indicate which abstract names of

motives were strongest for them. (Incidentally, I still think this list is as bad as ever I did, although it is still occasionally reproduced even now as an "experimental" study of motives.)

My conversion came about as a result of repeated demonstrations at the hands of Lazarsfeld, although I found later that P. M. Symonds[12] had been insisting on the same general principles for several years—and I presume *he* borrowed them from Aristotle! At any rate, Lazarsfeld insisted, in speech and in writing, that "*Why* is never a question; *why* is always a program"—and he began to produce evidence in support of this thesis. Put into other terms, he advised a study of human motivation by a series of interlocking and indirect questions which should, so far as possible, reproduce the time-line of the original behavior. Thus, the direct inquiry "Why did you come to this lecture tonight?" would, in all probability, elicit a series of nice, socially acceptable, and carefully rationalized responses. If taken at face value, these would leave us with an over-intellectualized view of the behavior involved, couched in terms of desire for self-improvement, interest in methods of the social sciences, plus perhaps a certain curiosity aroused by the idea that this talk was to deal with psychological problems in aviation.

Now suppose we take a much longer route and approach the problem indirectly. We may begin by asking what other lectures of the series you have attended; what others you plan to attend; whether attendance was required by registration in some course; what you usually do on Monday evenings; when you first began to attend psychological lectures and under what circumstances; in what company you came tonight—and so on. If the series of questions is well planned, there may begin to emerge certain characteristic patterns of behavior which cast a good deal of light on what would otherwise be regarded as a virtuous effort at self-improvement.

Thus, in a somewhat analogous study of attendance at the movies, it was found that certain members of the audience

[12] Symonds, P. M. Diagnosing Personality and Conduct. 122–173. Appleton-Century. 1931.

were night-habituated; that is, they always attended on Tuesday night. Another group were star-followers, who made an intellectual virtue of seeing Mae West in all of her dramatic triumphs. A third group represented an escape-from-home pattern, selecting the movies as a convenient way of satisfying an urge to remove themselves from the domestic scene. A fourth group were present under the urge to witness a specific film. A fifth group found in the movies a convenient way of wasting requisite amounts of time between other activities. Those of you who are regular attendants yourselves need not be reminded that a sizable group gave evidence that the moving picture theatre provided for them a surrogate for the balcony scene.

This grouping of motives is by no means the only outcome that may be expected from properly conducted studies. One investigation, for example, showed that a major factor in the cessation of milk-drinking was the identification of milk by the adolescent as a baby-food. Another has sketched clearly the variety of motives effective in book-buying. Others have depicted motivational factors at work in delinquency, in the maintenance of industrial morale, and in the use of campus recreational facilities—to name only a few.

If you object that this technique is not new, I shall be forced to agree. The new thing about it, however, is its conscious application. A psychiatrist, with whom I have had extended conversations of late, informs me that members of his profession have always used the general technique of indirect questioning along the time line, but that, in his opinion, many have failed to realize exactly what they have been trying to do. What is novel to many people, I find, is the idea that the question "Why?" may best be asked by asking a series of questions beginning with the words "What?" and "When?" and "How?" Nor does it take much imagination to see that many respondents who would resist the direct inquiry "Why?" are quite willing to supply the desired information—without ever realizing what they are doing—if the more indirect form is employed.

It is obvious that my half-dozen statements have left unsaid far more than they have mentioned. I have spoken as if mail and face-to-face inquiries were the same, while it is clear they are not. Each has its peculiar field of usefulness and each its peculiar advantages. No mention has been made of the whole problem of motivating the respondent to reply, although much has been written on this topic. I may mention that H. G. Weaver[13] reported a 50 per cent increase in returns when he enclosed a duplicate copy of a questionnaire, and that he obtained more than three times as many returns when he told respondents that they did not need to bother to return the blanks at all. Nothing has been said about the difference between interviewers who report the gist of what respondents say and those who actually report *verbatim* answers—yet the difference in value to the investigator is enormous. Nor has any attention been paid to a most crucial point—that of training interviewers to record "incidental" remarks by respondents. Yet experience has shown that insight into many a problem has depended upon some chance remark not called for by the formal interrogatory. And so we might go on through a score of other points which bear directly upon the formulation and use of effective questionnaires.

In closing, may I say that I hope the technological nature of my examples has not closed the ears of any one in the audience to the principles involved. The selection of the examples was dictated by the activities with which I have been concerned. The applicability of the principles in non-technological fields is above question. The questionnaire will always bring in results involving a certain probable error. In the hands of those who are willing to use it as a rifle, rather than as a blunderbuss, however, it can provide valuable information about human behavior that must otherwise be denied to those who would have it. The speaker hopes that he may have made certain converts towards membership in the Rifle Club.

[13] Weaver, H. G. Consumer Questionnaire Technique. *Amer. Marketing Jour.* 1: 115. 1934.

SECTION OF ANTHROPOLOGY

FEBRUARY 26, 1940

DOCTOR JACK HARRIS, Columbia University, New York City: *The Position of Women in a Nigerian Society.*[1]

In West Africa are located the great native kingdoms of Ashanti, Yoruba, Dahomey and Benin. Here the political organization is highly formalized. There are a number of groups bordering on these which, while less formal in their political organization, share many traits of culture with these kingdoms.

The Ibo of southeastern Nigeria form one such group. Approximately 500 politically independent communities roughly similar to Ozuitem, the Ibo community in which the author conducted his research, comprise the Ibo-speaking peoples.[2]

It is a common characteristic of these West African peoples that even when they are formally organized along patrilineal lines the power and importance of women are emphasized. In the great kingdoms their position has been formalized in such institutions as the court of the queen mother and in such formal rôles as those of priestesses, "Amazons," the chief's wife and the queen mother.

Where the patrilineal pattern of patrilocal residence, bride price, secret societies from which women are excluded, and a preference of sons over daughters obtains, as among the Ibo, less formal but equally effective mechanisms for the expression of women's importance prevail.

[1] A number of references to the position of women in Ibo society may be found in Meek, C. K. *Law and Authority in a Nigerian Tribe.* London: Oxford University Press, 1937. However, since Meek's inquiries occurred immediately after the Aba Riots he found it difficult to make systematic or intensive inquiries on this point. See also Leith-Ross, S. *African Women.* London: Faber and Faber, 1939, for personal experiences and general statements concerning Ibo women.

[2] Field work in Nigeria during 1938–39 was made possible through a Fellowship awarded the author by the Social Science Research Council.

Ozuitem has a population of approximately 5500 people. It is considered a social entity because of descent from a common ancestor, sacrifice at common shrines, similar customs and group meetings at time of crises. Most Ibo communities are composed of a number of patrilineal sibs. There are 17 in Ozuitem.

The community is divided and sub-divided into a number of groupings in the following manner:

1. *The community.* All members count descent from a common ancestor.
2. *Sib Groups.* There are three of these in Ozuitem, but for practical purposes of sacrifice, warfare and communal work, two closely related groupings combine, so that there is a working dual division common to all Ibo social units.
3. *Sib,* composed of from one to twelve kindreds—approximately 100 to 1200 people—who count descent from a common ancestor.
4. *Kindred,* consisting of from one to five extended families, who have a more immediate common ancestor than that of the sib.
5. *Extended family* includes from one to about fifteen primary families, who have common descent from an ancestor more recent than that of the kindred.
6. *Primary family,* consisting of a man, his wife or wives and their children.

Each of these groupings has its own executive council. The community council draws its representatives from the sib groups and the sibs. The sib council draws its representatives from the kindreds, the kindred from the extended families, and the extended family council is composed of the adult males in each primary family.

Although men repeatedly emphasize that women are practically chattels, that they have no power other than that allowed them by the men, women have a system of courts and councils which directly parallels those of the men. These are organized on the basis of the kinship units of the women's husbands following the pattern of patrilocal residence.

Women married into one extended family, kindred or sib form groups which pass laws regulating the activities of women, organize their own work, conduct their own courts, impose and collect fines. There also are meetings of women on a community basis at times when their welfare is threatened as, for example, when the men's council passes a law detrimental to the interests of women, or when excessive rainfall is ruining their crops and remedial measures have to be considered. Rare combined meetings of men and women are also held during crisis situations which demand the participation of both sexes as, for example, when a major deity who influences the entire community demands sacrifice and the women's suggestions and financial contributions are as welcome as those of the men.

Throughout the assertion of their rights, implicit in most of their actions, and at the root of their solidarity and coloring their attitudes, is the stress women place upon their rôles in the society as food producers and child bearers. At certain times, this emphasis reaches the conscious level. To illustrate, during the Aba Riots (to be described later), the women were indignant that they, "the trees which bear fruit," were to be taxed. They proclaimed that they felt safe in marching upon the white officials, because they were women and women were mothers—mothers not only of black people but of white. How could the white men harm mothers?

This attitude is also explicit in the form of the curse employed by a woman striking a pestle on the fireplace where she cooks food. She mentions the name of the man who has wronged her, calls upon the female ancestors and continues: "This, that you have done to me, is not good. I did no harm to your mother nor to your people. It is I that bore you in my womb. It is I who gave birth to you. It is I who cook for you to eat. This is the pestle I use to pound yams and coco yams for you to eat. May you soon die!" This phrasing of the curse is general for all women, even for one who has not given birth. The woman assumes the rôle of Ibo

womanhood and is, in effect, saying: "It is we women who give birth to men. It is we women who feed men. How dare you do an injustice to a woman?"

Men and women share responsibility for feeding the family, but upon women falls the burden for a longer period of the year. Women grow 20 crops on their farms and are responsible for the feeding of their families for approximately eight months of the year. From September to the middle of December—from the harvest of yams to the harvest of coco yams—it is the formal custom for men to feed their families. But men grow only one crop, yams, and, during his period to feed the family, a man is often forced to rely upon women's crops, which he frequently must buy in the market from other women, even though his wife may have these crops stored at home. In theory, food responsibilities of husband and wife are mutually exclusive, but most Ozuitem couples coöperate. Yet the theory can be sharply invoked if the couple quarrel. The wife may cut off her husband's food supply and he may later retaliate by withholding hers during the formal periods or by refusing to give her land for the next farming season.

Most men admit that in these situations they are at a disadvantage for a number of reasons. If the husband refuses to supply yams and other food, the wife has a number of crops on her farm or store to which she can turn, but during the women's feeding period, men's one crop, yams, is still in the ground. To meet this exigency many men cultivate a plot of land devoted solely to women's crops. This is called "farm of man who has no mother." The name derives from the fact that it is usually to his mother that a man turns, or, lacking these expedients, his brother's wife or his mistress. He has one other alternative, viz., he may cook his own food and take his wife's crops from her farm or store for this purpose (to which he has a theoretical right), but this exposes him to the sharp ridicule of the women and even of the men.[3]

[3] It is often pointed out in West African literature that women control the cooking pot, but, to my knowledge, there is no reference to women's control of the

The husband is careful in the exercise of any of these retaliatory measures since they may break up the marriage. The wise husband usually uses them as threats and rarely brings them into play.

The power of the ancestors in West Africa has often been discussed but this usually refers to the male ancestors. Such formal devices as calendrical sacrifices and shrines, characteristic of the male ancestors, are absent from the women's relationship to the female ancestors. Nevertheless, a great source of women's power in this area is derived from the female ancestors. For example, during a period of severe rainfall dangerous to the growth of their crops, women invoke their ancestresses and use fern-covered sticks as improvised symbols. With these sticks they draw lines across every path leading into Ozuitem to kill those human rain-makers who may be causing the excessive rainfall. During the Aba Riots, as well as at other crises, they gathered and sacrificed to these sticks to bring death to their enemies.

During women's council or court meetings, witnesses who make crucial statements must swear upon a pestle bound with a young palm leaf which derives its power to kill those who swear falsely from the female ancestors. As we have seen, a woman gravely wronged by a man may strike her pestle on her fireplace and call upon the ancestresses to kill the man.

In situations where a woman theoretically has few rights, a number of mechanisms are employed to circumvent this fact. While examples could be chosen from, among others, secret societies or the marriage situation, the following will serve to illustrate the point. Theoretically, a woman has no title to property. However, a woman can buy land or get it on pledge, but she needs a male proxy, either her husband or a close male relative, usually of her husband's extended family. This man retains formal title to the land, but this is

actual food supply. In addition to the Ibo, I have determined that a form of this control obtains among some northern Ibibo groups and Prof. C. Daryll Forde has communicated to me that an attenuated form functions among the Yakö, a semi-Bantu group on the Cross River.

only nominal. A woman may also frustrate her husband's control over her by buying or receiving land on pledge through a lover, so that the husband has no rights over this land. Furthermore, since the husband neither cuts nor burns the bush on this land, his theoretical right to the crops grown on it is effectually lessened.

The articles which the woman brings to her husband's compound at marriage belong to the husband. He purchases these household goods in a ceremonial transaction at only a fraction of their worth, symbolizing the fact that even these things do not belong to the woman and she comes to her husband with no possessions of her own. However, in case of divorce, a woman may take a number of things with her, along with a few head of sheep, through the force of her own personality and the strength of her relatives. Only in a few cases does the wife leave with no property.

Women have a number of means of exerting pressure upon men, when they feel that they have been wronged or when they wish their demands met. Such techniques as boycott, strike and, as we have seen, ridicule and cursing, are employed. A few examples illustrate some of these techniques.

1. The men of one sib were very angry because their wives were openly having relations with their lovers. The men, their pride hurt and afraid that the ancestors might cause their wives to be barren because of this flagrant adultery, met and passed a law to the effect that every woman in the sib should renounce her lover and present a goat to her husband as a token of repentance. At first, the older women were exempt from this penalty, but they directed so much ridicule toward the men that they too were included in the order. The women held small secret meetings and, a few mornings later, they went to a neighboring sib, leaving all but suckling children behind them. The men had to care for the crying children and were forced to carry water, bring firewood and to do the rest of the women's work. They endured it for a day and

a half and then, they went to the women and begged them to return. They paid the men of the other sib to intercede in their behalf. To the women, the men gave one goat and apologized informally and formally. The women returned.

2. Women of a kindred had repeatedly asked the men to order the proper age grade to clean an overgrown path to their farms and to repair a bridge. The men neglected to do this and the women refused to cook food for their husbands until this order was carried out.

3. The following incident illustrates the boycott technique. A woman refused to pay a fine which the women's council had imposed. All the other women refused to speak to her, to buy from, or sell to her in the market, and to give her live embers for her fire. Any woman who violated this boycott was to be severely fined. Within three weeks the errant woman paid her fine.

4. There is also the technique of "sitting upon a man." To the compound of a man who has wronged them, the women come singing songs of ridicule and remain there, sometimes staying overnight and longer, until the man capitulates.

As a final example of the power and importance of women, we refer briefly to the organized protest of Ibo women known in Nigeria as the Aba Riots of 1929.[4] In addition to the strong intra-community unity of women the Aba Riots were a striking illustration of women's ability to organize upon an inter-community basis, never achieved by men. In answer to a rumor swiftly spread through the market system that women were to be taxed, and quickly responding to a hurried call for concerted action in face of a common danger, more than 10,000 women drawn from various communities over a wide area marched upon the headquarters of the white administrators. They demanded concrete assurance that taxes would not be levied upon them as they were upon Ibo men two years before. The women carried with them the fern-

[4] For a description of these riots see Perham, M. *Native Administration in Nigeria.* London: Oxford University Press. 1937.

covered sticks, symbols of the protection of the female ancestors. Spokeswomen and leaders appeared to present the demands of these women from various communities. The revolts spread and culminated in a violent reaction against white acculturation.

Although, theoretically, this is a patrilineal society and formal controls are in the hands of men, as has been shown, the women exercise considerable power in their councils and courts, in their rôles as child-bearers and food-producers and through the religious sanction of the female ancestors. Even where women have no formal methods of gaining their ends, they can resort to such ingenious techniques as ridicule, boycott and strike. In addition to their power within the community, women have developed a mechanism for "international" solidarity, as displayed in the Aba Riots, which the men have never achieved.

SECTION OF PHYSICS AND CHEMISTRY

FEBRUARY 2 AND 3, 1940

CONFERENCE on "Physical, Physical-Chemical and Organic-Chemical Evidence Regarding Crystalline Protein Molecules."[1]

Professor Edwin J. Cohn, Department of Physical Chemistry, Harvard University, was in charge of this meeting as Conference Chairman. The program consisted of the following papers:—

"Evidence from Organic Chemistry Regarding the Composition of Protein Molecules," by Doctor H. B. Vickery, Connecticut Agricultural Experiment Station. Discussion—M. Bergmann, R. K. Cannan, H. T. Clarke, E. Brand, V. du Vigneaud, J. S. Fruton, M. Heidelberger, S. Moore, H. B. Lewis, T. L. McMeekin, R. Schoenheimer, W. H. Stein.

"Evidence from Physical Chemistry Regarding the Size and Shape of Protein Molecules. Ultracentrifugation, Diffusion, Viscosity, Dielectric Dispersion and Double Refraction of Flow," by Doctor J. L. Oncley, Harvard Medical School. Discussion—J. H. Bauer, J. T. Edsall, J. D. Ferry, R. M. Fuoss, J. G. Kirkwood, E. O. Kraemer, M. A. Lauffer, J. W. Mehl, H. Mueller, H. Neurath, J. H. Northrop, L. Onsager, E. G. Pickels, G. Scatchard, W. M. Stanley, J. W. Williams.

"Evidence from X-Rays Regarding the Structure of Protein Molecules," by Doctor B. Warren and Doctor I. Fankuchen, Massachusetts Institute of Technology. Discussion—D. Harker, M. L. Huggins, I. Langmuir, C. E. Mooney, A. L. Patterson, D. M. Wrinch, R. W. G. Wyckoff.

[1] Only molecules that appear to be chemical individuals and that have been thus far studied by several methods were considered. These included egg albumin, lactoglobulin, insulin, hemoglobin, chymotrypsin, serum albumin and edestin.

NEW MEMBERS

Elected February 5, 1940

SUSTAINING MEMBER

Joyner, A. L., M.D., Director, Immunological Research, Lederle Laboratories, Pearl River, N. Y.

ACTIVE MEMBERS

Ackermann, Alfred J., M.D., Instructor, Radiology, School of Medicine, University of Oklahoma, Oklahoma City, Okla.

Akin, Russell B., Ph.D., Research Chemist, E. I. duPont de Nemours Co., Arlington, N. J.

Asch, Solomon E., Ph.D., Assistant Professor, Psychology, Brooklyn College, Brooklyn, N. Y.

Bolling, Diana, B.S., Research Fellow, N. Y. State Psychiatric Institute and Hospital, New York, N. Y.

Brescia, Frank, Ph.D., Instructor, Chemistry, College of the City of New York, N. Y.

Collins, James A., M.B.A., Statistician, New York, N. Y.

Coren, Malvin M., M.D., Instructor, Anatomy, New York Medical College, New York, N. Y.

Evans, Mary J., Ph.D., M.D., Instructor, Bacteriology, Women's Medical College, Philadelphia, Pa.

Johnson, Rupert H., A.B., West Point graduate, Investment Broker, New York, N. Y.

Hardy, James D., Ph.D., Research Associate, Russell Sage Institute of Pathology, New York, N. Y.

Hawthorne, Margaret J., B.S., Instructor, Supervisor, School of Nursing, Presbyterian Hospital, New York, N. Y.

Landry, Herbert A., Ph.D., Research Assistant, Board of Education, New York, N. Y.

Lehman, Robert A., Ph.D., Instructor, Therapeutics, College of Medicine, New York University, N. Y.

Novalis, Nicholas Arthur, M.D., Instructor, Anatomy, New York Medical College, New York, N. Y.

Parker, Raymond C., Ph.D., Head, Virus Division, Biological Laboratories, E. R. Squibb & Sons, New Brunswick, N. J.

Pignatelli, Myrtle E. L., M.A., Clinical Psychologist, Bellevue Psychiatric Hospital, New York, N. Y.

Scarff, John Edwin, M.D., Assistant Professor, Clinical Neurological Surgery, Columbia University, New York, N. Y.

Smith, Emil L., Ph.D., Research Fellow, Physiological Chemistry, Yale University, New Haven, Conn.

Stein, William H., Ph.D., Assistant, Biochemistry, Rockefeller Institute, New York, N. Y.

Temple, William J., Ph.D., Instructor, Speech, Brooklyn College, Brooklyn, N. Y.

Thomas, Bernard O. A., D.D.S., Research Assistant, Histology, Columbia University, New York, N. Y.

Trask, James D., M.D., Associate Professor, Pediatrics, Yale University, New Haven, Conn.

Urey, Harold Clayton, Ph.D., Sc.D., Professor and Executive Officer, Chemistry, Columbia University, New York, N. Y.

Warfel, Herbert E., M.S., Assistant Professor, Zoology, University of New Hampshire, Durham, N. H.

Wetmore, Alexander, Ph.D., D.Sc., Assistant Secretary, Smithsonian Institution, in charge U. S. National Museum, Washington, D. C.

Zahl, Paul Arthur, Ph.D., Research Associate, Haskins Laboratories, Schenectady, N. Y.

ASSOCIATE MEMBERS

Ashley, Samuel E. Q., M.A., In charge, Analytical Laboratory, General Electric Co., Pittsfield, Mass.

Biberman, Lucien M., Student, Rensselaer Polytechnic Institute, Troy, N. Y.

Bowles, Edgar, Ph.D., Geologist, Geological Survey of Alabama, Tuscaloosa, Ala.

Cook, Roy Selden, Ph.D., Professor, Chemistry; Head, Science Dept., Mary Washington College, Fredericksburg, Va.

Derów, Matthew Arnold, M.D., Instructor, Bacteriology and Immunity, School of Medicine, Boston University, Boston, Mass.

Hamilton, J. Bruce, A.B., Reference Assistant, Science Dept., Boston Public Library, Boston, Mass.

Renaud, E. B., Ph.D., Professor, Anthropology, University of Denver, Denver, Colo.

Scatchard, George, Ph.D., Professor, Physical Chemistry, Massachusetts Institute of Technology, Cambridge, Mass.

Steele, N. Dudley, M.S., Chemical Engineer, Godfrey L. Cabot, Inc., Pampa, Texas.

van Diest, Alice E., M.A., Professor, Sociology and Social Work, Colorado College, Colorado Springs, Colo.

Waldo, Allen W., Ph.D., Assistant Professor, College of the Pacific, Stockton, Calif.

Waugh, Karl T., Ph.D., LL.D., Personnel Officer, Dept. Public Assistance of Pennsylvania, Harrisburg, Pa.

Welch, d'Alte A., Ph.D., Zoology Department, Johns Hopkins University, Baltimore, Md.

Winder, Claude V., Sc.D. Instructor, Physiology, University of Michigan, Ann Arbor, Mich.

Zapffe, Carl A., Sc.D., Research Associate, Battelle Memorial Institute, Columbus, Ohio.

STUDENT MEMBERS

Osterud, K. L., A.B., Graduate Assistant, Biology, New York University, New York, N. Y.

Parke, Roger I., M.A., General Science Instructor, New York School for the Deaf, New York, N. Y.